Thus Sachs is copied even to including the cyme in *monopodial* systems. Surely this is a contradiction in terms, and might be avoided by the use of "lateral," in contradistinction to "dichotomous." In the figures (17, 19) of uniparous cymes, Sachs, and with him Prantl, omit to mention that the diagram is taken in *plan*, a point the more important that in French and English text-books it has been usual to give such diagrams of inflorescence in *elevation*. Without noticing this, a trap is laid through which not students alone have fallen into the error of thinking that the Germans use "scorpioid" and "helicoid" in senses inverse to the usage of other botanists. Under inflorescence no mention is made of the very useful French "Cymobotrya" terminology, possibly through feelings of patriotism, with which, however, Englishmen are not concerned. The Elder is given as the example of a corymb; which term is, however, restricted by the best botanists to the corymbose *raceme*, of which the elder is not an example.

In the histology there are several not unimportant errors, probably Prantl's own. He says that the phloem contains both "phloem parenchyma" and "cambiform tissue"—is not phloem parenchyma always (primitively at least) cambiform? We are told (p. 51) that the vessels of secondary wood are "*invariably* provided with *bordered* pits;" this is far too absolute. Under collenchyma no mention is made of its commoner form, distinguished as "concave" by Vesque. Endoderm is defined as peculiar to Dicotyledons! Under "stomata" no mention is made of water-pores. The account of the structure of roots and the development of their secondary wood in Dicotyledons is hardly explicit enough, and almost demands the introduction of one or two pure diagrams; and when it is stated that rootlets arise in front of the xylem bundles of the root, mention should be made of such important exceptions as Umbellifers and Grasses.

The physiology proper is singularly well treated, though perhaps with too great a fear of detail. Thus no sufficient account is given of the *vis a fronte* and the *vis a tergo*, which lead to the movements of the rising sap.

A few little mistakes have been left uncorrected in the systematic part. The legume is stated on p. 197 to occur in "all the Leguminosæ;" and while this is modified in the account of the order on pp. 278-280, a true legume is here implicitly denied to the Cæsalpinieæ! "Replum" is given as meaning a false-dissepiment of the Crucifers, a use unauthorised by the best systematists, and inconsistent alike with its application to the lomentaceous Leguminosæ and to its Latin signification.

The figures are good, but, as usual in English editions of foreign works, poorly printed. The worse fault of separating them widely from the text they illustrate has been avoided.

Finally, despite all trouble taken by the editor, oversights will occur in a translation. Thus *Tüllen* is given in italics without its English (?) equivalent, "tyloses," and "bracteole" is given instead of the more familiar "bractlet." But these blemishes show how good is the book in which they are the worst to be found; they have been here put forward chiefly in the hope of helping the editor in the new edition which will soon be demanded; and it is with a safe conscience that we would recommend this book as the best of its kind in the English language.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Ocean Circulation

THE notice in NATURE (vol. xxii. p. 207) of the experimental researches of Professors Haughton and E. Reynolds on the coefficient of friction of water upon water, having concluded with the statement that "the authors of this research point out that these results tend to negative the theory of Dr. Carpenter that the phenomena of ocean-circulation are due to the greater height of the water at the equator as compared with that at the poles," I must be allowed to protest against being credited (or rather *dis*credited) with a doctrine which is neither expressed nor implied in anything I have written on the subject.

The doctrine which I *have* advocated is no other than that first distinctly promulgated by Lenz in 1847, and now accepted by numerous Physicists of the highest eminence, both British and Continental; viz., that besides the *horizontal* circulation produced by the action of winds on the ocean-surface, there is a *vertical* circulation of which Polar cold is the *primum mobile*, consisting of an *under*flow of Polar water (chiefly from the Antarctic area) towards and even beyond the Equatorial zone, and a complementary *upper*-flow of Equatorial water towards the Poles.

That every part of the vast Oceanic basin in free communication with either of the Polar areas is occupied, to within the range of the surface-heating produced by insolation,[1] by water which has been cooled down in one of those areas, is now one of the best-established facts of Terrestrial Physics. And those who cannot find in the excess of specific gravity imparted to sea-water by Polar cold, an adequate cause for this movement of translation, are bound to account for it in some other way.

I venture to submit to the accomplished professors of Trinity College, that laboratory experiments made to determine the friction of water upon water *at sensible velocities* can scarcely prove that when the equilibrium of a great mass of water has been disturbed, there will *not* be *any* movement of translation (however slow) for its recovery. And I would suggest to them that they should rather investigate the conditions of one of those "experiments ready prepared for us by Nature," which is constantly going on in the Baltic Straits, and of which the results have been for many years past most carefully recorded by Dr. Meyer of Kiel and his associates. Four factors are there in continually varying action, viz. (1) difference of *level* between Baltic and North Sea water; (2) difference of *salinity*; (3) difference of *temperature*, mainly due to an importation of Polar water into the Skager-rack; and (4) *surface-movement* produced by wind, which may also modify the relative levels.

I am assured by Dr. Meyer that the action of each of these factors has now been so fully determined, that the effect of any combination of them can be predicted as certainly as ordinary tidal phenomena. And of the competence of small differences in specific gravity to produce movement in great bodies of water, no one who has investigated the question on the great scale seems to have the smallest doubt. This was the unhesitating conviction of the late Mr. Froude, as the result of his numerous observations on harbours, lochs, and fiords, communicating with the sea at their mouths: for he assured me that wherever the salinity of the water at their upper end is lowered by the descent of fresh water from the land, producing a slight

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[1] The researches of Prof. Forel and his associates on the Swiss lakes clearly show that in *fresh* water the heating effect of insolation is limited to about 100 feet. In *salt* water, on the other hand—as I pointed out in my Mediterranean Report—there is a *downward convection* of heat produced by the sinking of the water made heavier at the surface by saline concentration. In the Mediterranean, where this effect is limited to a part of the year, it scarcely shows itself below 100 fathoms (600 feet); but under the Equator, where it is constant, the surface-heated stratum ranges downwards to from 300 to 400 fathoms. Beneath this depth the thermometer progressively sinks in the ocean-basin generally (the thermal condition of the North Atlantic being altogether exceptional) from 40° to 33° or thereabouts; whilst in the Mediterranean, to the deeper part of whose basin the Polar underflow has no access, the thermometer shows a uniform temperature of from 54° to 56° (according to the locality) from the surface-heated stratum to the deepest bottom (2,000 fathoms).

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surface outflow, he could trace an underflow of sea-water up the channel; and this he could attribute to nothing else than the slight excess of *downward* and therefore *lateral* pressure in the *outside* column, depending on the continually-maintained reduction in the mean salinity of the *inside* column, which more than compensated for any slight excess in its level.

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The Freshwater Medusa

IN NATURE (vol. xxii. p. 190) Prof. Lankester refers to a statement of mine in the preceding number, that I had arranged with Mr. Sowerby some methods of observation from which I hoped to obtain data for the determination of important points regarding the development of the freshwater Medusa, and expresses a desire to be informed as to the nature of the proposed methods.

The obvious and only practicable course to be adopted with this view was arranged with Mr. Sowerby by Mr. Busk and myself, and consisted in the separation of specimens from the Victoria tank and their confinement in glass jars, which, in order to secure a continuance of the necessary temperature conditions, were to be retained in the same house with the tank in which the Medusa had shown itself. The examination from time to time of these jars would probably bring to light facts having a direct bearing on the development of the animal. This method of observation, indeed, is so obvious that it must have occurred to any one engaged in the investigation it was designed to aid.

Prof. Lankester now says that Mr. Sowerby informs him that he had undertaken no experiments except such as had been carried out at his request; but as it seems that these are identical with those proposed by Mr. Busk and myself, nothing has been thereby lost.

Residing at a distance from London, my opportunities of studying the life-history of the Medusa are at this moment comparatively few. Prof. Lankester, however, being on the spot, and having an unlimited supply of subjects for investigation, will doubtless avail himself of the advantage thus afforded, and will render our knowledge of this remarkable little animal more complete than would otherwise have been possible.

Prof. Lankester refers to the difference of opinion between himself and me, and promises to bring proofs of his own views. When these proofs are offered I shall gladly accept them. My desire is that no previous expression of opinion shall blind me to evidence in favour of a contrary position. The only important points, however, on which my conclusions have been absolutely at variance with those of Prof. Lankester are the presence of a circular canal and the perviousness of the distal extremities of the radial canals. With regard to these there cannot in my opinion be the slightest doubt.

The nature of the marginal bodies is also a point of much importance in this investigation, but I have expressed only a conditional opinion with regard to it. While Prof. Lankester considered these bodies as undoubtedly tentacular, I held that the evidence afforded by adult and by comparatively young specimens is in favour of their velar origin; but at the same time I stated that this point cannot be decided without the evidence obtained from development.

I also drew attention to the remarkable attachment of the tentacles, whose adnate basal portion occupies exactly the position of the *peronia* in the Narcomedusæ and Trachomedusæ, but I failed to find evidence of the presence of true peronia as described by Prof. Lankester, who now admits that the peronia while present are rudimental.

The other points, namely those which concern the systematic position of the Medusa, are necessarily only hypothetical. It appeared to me that while there are certain features in the structure of the adult Medusa which point towards the Trachomedusæ, there are others which connect it with the Leptomedusæ, to which on the whole it seemed to be more closely allied, though holding a position intermediate between the two; but I regarded the data in our possession as insufficient for the final determination of this point, which can be absolutely settled by the study of development alone.

Prof. Lankester promises details of his observations in this month's number of the *Quarterly Journal of Microscopic Science*, and I look forward to what I doubt not will be a valuable contribution to hydroid zoology.

As to the name of the Medusa, Prof. Lankester, while abandoning his generic name in favour of mine, declares it to be his intention to retain his own specific name for the animal. This is to me a matter of complete indifference. Science can gain nothing from personal contention about names, and the time so occupied might with far greater advantage be devoted to more useful and lasting work.

J. ALLMAN

On the Simplest Continuous Manifold of Two Dimensions and of Finite Extent

SO far as I am concerned Mr. Frankland answers too soon (p. 170), for I am sorry to say I have not read Klein in the meantime. Therefore my reply is provisional. A hint was given of Mr. Frankland's explanation by Mr. Newcomb in a phrase quoted by Mr. Halsted (*American Journ. of Math.*, I. iii. 275, paper on the bibliography of hyperspace, &c.): "The first elements of complex functions imply that a line can change direction without passing through infinity or zero." We do not require even the first elements of complex functions to tell us that we can get to the other side of a point without passing through it, provided we can go round it. But the question was not whether "a line" simply could be thus reversed, but whether it could be so with the geodetic perpendicular in question described in a uniform continuous manifold of two dimensions. Mr. Frankland's explanation expressly takes account of a third dimension. It supposes the moving line to generate a sort of skew helicoid about the fixed line to which it is perpendicular. But how can even initial portions of successive generators be in the same plane, Euclidean or other? This point may seem incidental, but I think it is essential, so I omit further questions.

Somewhere in his "Dynamic" Clifford says that Klein's double surface is a sphere in which opposite points are considered as one. In this light the mystery disappears. There are two perpendiculars: considered as one they never change sign; because, considered as two, they periodically exchange signs. But if opposite points do not coincide, they may be "one," but they are not one point; if they do, is the manifold they compose a surface? Mr. Frankland has not called it a surface: but is it continuous?

There is a very well-known manifold which obviously obeys the laws worked out by Mr. Frankland and Mr. Newcomb, a system of straight lines, not vectors, through a common point; or, reciprocally, a system of planes. To measure of curvature answers density; if this is constant, the geodetic distance from a point to a geodetic line is represented by the angle between a straight line and a plane.

It may be worth while to note one or two oversights in the writing or printing of Mr. Frankland's letter. For $\frac{1}{2}l\sqrt{-1}$ we ought to have an expression involving the angle between the geodetics. The sentence "If a being," &c., is a quotation, and the last word should be "position," not "poise."

Both Mr. Newcomb and Mr. Frankland understand my intention as more negative than it was. I said (xv. 547) "it could hardly fail to be instructive if Mr. Frankland would explain," &c. Probably I underrated the difficulty, in this Euclidean world, of making it clear that one means just what one says.

C. J. MONRO

Hadley, June 29

A Fourth State of Matter

IT seems to me that Mr. Tolver Preston in his letter on the above to NATURE (vol. xxii. p. 192) has somewhat overlooked the context in the objections he urges against Mr. Crookes's remark that "an isolated molecule is an inconceivable entity." It is plain that Mr. Crookes meant this statement to apply to the *quality*, not the *existence* of a molecule, and granting Mr. Crookes's premisses regarding the constitution of matter, it appears a very fair deduction; since if the three states of matter (as we know it), viz., solid, liquid, and gas, owe their different *qualities* merely to different modes of motion of the ultimate molecules, it is quite conceivable as well as logical to suppose that the latter have a nature totally unlike that of the effects of their motion, and therefore inconceivable to us by reason of its dissimilarity to anything of which we at present possess any knowledge.

Again, with reference to the remark, "solid it cannot be,"

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